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SOME UNRECOGNISED LAWS OF NATURE.

Some Unrecognised Laws of Nature. An Inquiry into the Causes of Physical Phenomena, with Special Reference to Gravitation. By Ignatius Singer and Lewis H. Berens. With illustrations. Pp. xvi + 483. (London: John Murray, 1897.)

"AT last, after years of patient plodding in dim regions, where the footprints are few and the pitfalls many, the time has arrived when we are enabled to place before the world of science the first-fruits of our explorations."

So say the authors, Messrs. Ignatius Singer and Lewis H. Berens, at the opening of the preface to a handsome volume of nearly 500 octavo pages, published under the above title by Mr. John Murray, and printed in good style by the printer to the University of Oxford.[1] A subordinate title tells us that it is "an inquiry into the causes of physical phenomena, with special reference to gravitation," and a cursory inspection shows that it is decorated with quotations from great writers and philosophers at the head of each chapter, and that it is altogether a most ambitious work, courting the widest and closest attention.

The present reviewer is not one to regard lightly the danger of summarily rejecting a germ of new discovery because it happens to conflict with orthodox opinions, nor has he failed to have borne in upon him the conviction that there are indeed many real laws of nature at present unrecognised, some unsuspected, others scouted, by contemporary science.

It is, therefore, in no spirit of levity that he finds it his duty to state not only that the present volume contains much that is plainly and simply trash, he has to state further that, so far as he has been able to judge, there is nothing in it of the slightest value from cover to cover.

The former of these statements it is easy to justify by quotation; the latter may very likely be considered by the authors as an unfounded individual opinion typical of the arrogance of every professed physicist with regard to any novel view of the universe which may be brought to his notice. It is, however, difficult to imagine what manner of men these authors can be, nor how it happens that writers so conspicuously ignorant of the elements of physics, beyond what they have culled from a superficial acquaintance with a few popular books, should yet think fit to overflow in a wordy and pretentious volume, controverting many of the best-established scientific truths, criticising lightly the acknowledged masters of science, and setting forth a quantity of false philosophy and misapprehended facts with a flourish as if a new Principia were being given to the world. Who, again, are the readers of such a book? It has been most respectfully received in some quarters. Can it be that the state of school education in England is such that any considerable number of men of letters or of philosophy, or any other branch of study, find themselves able to accept such a book as this as containing possibly as good and sound science as, let us say, Thomson and Tait? Neither work is by them really read; perhaps both works seem to them nearly on a par. Perhaps they think that since Thomson and Tait and Helmholtz and Newton are all adversely criticised in the work before us, it is very likely true that the pretended facts of our boasted science are all uncertain fictions, and that the authorised scientific version of to-day is little better than a mass of hypothetical dogma.

If any such suspicion is in the slightest degree possible among the members of so-called educated society, it means that we are, after all, not far away from the dark ages; the death of a comparatively few individuals would perhaps loosen the grip of the human race on the truths of science, and plunge us once more into pre-Galilean ignorance. It is to be wondered if there are not more than a few who would welcome the change.

If the book is really an isolated phenomenon, it is as insignificant as the customary production of those paradoxers who were so delightfully gibbeted years ago by De Morgan; but it has not the usual air of the work of the paradoxer: the first half, at any rate, may easily impose upon the unwary as being a serious and sober attempt to solve perplexing problems, and contribute to the progress of natural knowledge. Sober and serious in intention we doubt not that it is, we do not suppose that the authors are playing off a conscious and expensive hoax; but, without the slightest desire to be abusive, we must maintain our view that a very small ingredient of common modesty would have prevented their shouting out their ignorance to the world—would have withheld them, with their little pittance of popular knowledge, from "undertaking to write the whole body of physicks," as Montaigne has it.

However, it is only fair now that we give some idea of the scope of the book, and that we make from it some illustrative extracts, so as to justify the terms of strictly measured depreciation which we have been constrained to use in our attempt to estimate it truly.

The four great fundamental and universal laws of nature, according to Mr. Singer (we are told in a note at the end of the preface that the science and philosophy of the work are exclusively due to Mr. Singer), are Persistence, Resistance, Reciprocity, and Equalisation.

The law of persistence is thus worded: "All matter tends to persevere in whatever state it may happen to be, and to resist change"; merely a simple and vague parody of part of the first law of motion, but it is intended to be much more comprehensive, and to include among other things such a statement as the following: "All organisms have a tendency to preserve their structure, organs, functions, habits, and dispositions unchanged."

As to "Resistance," we are told that it is the same thing as Persistence, and constitutes its quantitative measure; so the law of resistance, if it were to be stated definitely, would become a part of Newton's second law. The authors, however, rather avoid definite statements, and prefer nouns in capitals or italics, as if such nouns were laws of nature. By "Reciprocity" seems to be meant something akin to the third law of motion, though that is not in the least understood by them; and by "Equalisation" is intended something like the second law of thermodynamics, viz. that unless a difference of state exists between two bodies, there is no activity or tendency to activity.

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[1] We do not hereby intend to imply that the University of Oxford is responsible for, or cognisant of, the production; but we do think that some care should be taken not to let its imprimatur be even apparently affixed to such a book as the one before us.

Thus it may be admitted that the customary teachings of science have not been wholly thrown away upon Messrs. Singer and Berens; they have acquired some rudimentary notions from their perusal of popular expositions of science, but the amount of vagueness and inaccuracy with which they have contrived to shroud those notions is almost incredible except to those who take the trouble to read their book.

In the first place, they are careful to confuse their terminology, as the following explanatory notes testify:—

> "We use the terms force and energy synonymously, and in the sense of power or strength, and shall use either of these indiscriminately as the one or the other may happen to be most convenient (p. 49).
>
> "To us 'heat' means simply temperature." Work can be got from a body by either heating or cooling it, and it is the degree of difference of temperature that is important in connection with the mechanical equivalent of heat, not heat *per se* (pp. 86, 88).
>
> "Pressure is 'work,' and so is motion" (p. 106).
>
> "'Force' and 'mass' (or 'weight' and 'mass') are synonymous terms; two different names for one and the same thing." And yet the proportionality of weight and mass or of gravitation force and mass is put forth as a real fact of the Newtonian theory (p. 349).

In the second place, they ignore in detail the laws which they admit in general. For instance, on p. 77 occurs the following illustration:—

> "Two bodies attracting each other, the one with a force of 100 and the other with a force of 10, the stronger body would be pulled one-tenth and the lesser nine-tenths out of their respective positions."

And many other instances might be given of their entire misapprehension of Newton's third law.

There are many singular instances of what appears to be malevolent criticism, statements of the beliefs or usages of orthodox science which are utterly destitute of basis in fact. The following may serve as illustrations:—

> "Confining our attention to what are called the physical sciences, we still find the greatest possible differences in both language and conception. We have hydraulics and hydrostatics, mechanics, dynamics and pneumatics, electricity and magnetism, light, sound, and heat, and so forth; each separate branch of knowledge having its own distinctive theory and peculiar terminology not applicable to the others. It is like the confusion at the Tower of Babel. The workers in the different fields of knowledge cannot understand each other, therefore cannot commune with each other for the purpose of seeing whether the conclusions deduced from one group of facts agree with the conclusions derived in other fields of inquiry."
>
> "The close relation of the different phenomena embraced under electricity, magnetism, and galvanism is now well known, yet are they still treated as separate and distinct sciences."

On p. 39 it is asserted that Newton's pendulum experiments, whereby he proved that inertia and weight were proportional, were proofs of nothing whatever, being only equivalent to using first one balance and then another. And on p. 40 it is said that when Newton found that pendulum bobs composed of two different materials swung together if their centres of oscillation agreed, he must have adjusted their centres of oscillation experimentally by means of equal times of swing.

In other words, he was either a fool or a knave; the full-blown paradoxer would not have shrunk from the appropriate epithets. But our authors are more polite; they are good enough to say, "Our quarrel is not with Sir Isaac Newton, but with human frailty"!

> "The fixing of the absolute zero at − 273° C. is based on the theory that at that temperature a body would practically be annihilated—a conclusion which in a previous chapter we have shown to be unwarranted by the facts"!

There is more about this absolute zero of temperature, which is evidently a bugbear, and is considered as nonsensical as absolute position or absolute truth; in fact, on the subject of thermodynamics generally the authors are naturally afflicted with wholesale ignorance, which they believe to be scepticism. "The action of the steam engine is generally attributed to heat," they say on p. 84, "and calculations are based as to the quantity of work to be got out of a heat engine on the supposed mechanical equivalent of heat," and they proceed to point out the errors in this view. It appears that "the true cause of the motion of the piston is a disturbed equilibrium, and consequent tendency to equalisation." "Heat is no more an entity than acidity or hardness or depth of colour." In fact the whole doctrine of energy is absurd.

> "The whole doctrine of 'energy,' with all its astounding and contradictory corrollaries, has not been deduced from any facts at all, but is begotten of those conceptions which have come down to us in unbroken succession from our primitive and ignorant ancestors. The facts have merely been distorted in order to make them fit into the mould of the prejudiced human mind."

After this they proceed to "deal with the error which has given rise to the doctrine of the dissipation of energy." "When estimating the efficiency or power of doing work of a body we do not measure the *force which is just necessary to arrest its motion* [as we ought to do, is implied], but that which actually causes it to move a certain distance; in which case there is bound to be a discrepancy, an apparent loss."

Joule's experiments involved also a fundamental error, "owing to his assumption that a certain weight descending a certain height in a certain time is the measure of its mechanical power."

> "Where he committed the error was that he assumed the whole power of the descending weight to have been expended in driving his mechanism, [but] to produce motion in the mechanism the weight had to be greater than the resistance of the latter, . . . The power of the weight was greater than the mechanical effect in his paddles, and hence *greater than the corresponding thermal effect*. But *by how much greater* Joule never thought of determining."

No, alas, he believed practically as well as theoretically that action and reaction were equal.

> "Instead of estimating the heat' produced by friction, and then dividing the assumed mechanical power by the number of heat units—as Joule did—he might have estimated the mechanical effect produced by 'heat,' and

> en have calculated the heat equivalent of work by viding the calculated thermal units (which no doubt he ould have assumed as being all converted into echanical effect) by the resultant work expressed in ot-pounds. Manifestly he would, in the latter case, ve given too high a heat value to the mechanical effects; st as in his actual reasoning he has given too high a echanical value to his heat units."

And then in a footnote:

> "There can be no doubt that it was purely accidental at the latter process has not been adopted by Joule"!

If he had happened to make his experiments in this verse way, the authors go on to say, the law deduced ould be "that the heat energy of the universe is coming every day more and more changed into echanical energy."

> "Thomson's error was due to his acceptance of Joule's nclusions. Joule, in his turn, arrived at his fallacious nclusions by the double error, . . . and both these ilosophers have shared in the fundamental error that nergy' is a *something* which is transferred from one dy to another. Thus do we see how fertile an erroneous nception is in producing error. . . . Verily, as Lord acon expressed it"—

Then follows a quotation about the tendency of the man mind to reject contrary instances—not without reat and pernicious prejudice—"in order that the thority of those previous conclusions may remain unhaken."

But not only do they thus misapprehend matters f real physics: the authors also fail to understand ven such simple popular phrases as Tyndall's "Heat mode of motion," and criticise it as if "mode" eant "result": saying that if heat were due to otion, then equal weights of different substances lling from equal heights should manifest the same increase of temperature (p. 165), and they go on to say that periments on this point would be desirable. The eneration of heat, they say, is due to states or acts of oercion, and in one of the few modest sentences to be und in the book they confess, on p. 163, that

> our own belief is that all bodies while in states of ercion [like compressed air] are constant sources of eating, but that the diffusion is equal to the rate of eneration, and hence no sensible increase of temperature can take place."

Further on they recover from this unusual weakness, toutly maintaining that what they call Tyndall's view of eat is quite wrong, "since heat is due to arrested motion, ot to motion itself," and boldly asserting that the amount f heat generated by bodies falling from a height depends pon their hardness,

> a quantity of water or mercury falling from a certain eight would not generate as much heat as would a like uantity of, say, steel falling from an equal height."

It would be tedious to follow the authors through their ild statements concerning electricity and magnetism in letail: the following brief extracts must suffice.

> "The gold-leaf electroscope, consisting as it does of lass and metal, will be found to be analogous to an lectric machine. Indeed, the principle of the instrument is not yet understood, and no attempt has been ade to explain it" (p. 211).
>
> "The resistance of thin wires is less than the resistance of thicker wires of the same material, which is the opposite of what is currently believed to be the case" (p. 222).
>
> "The total resistance of 6 lbs. of copper, for instance, would be the same whether the 6 lbs. of copper were only a yard in length, with a corresponding diameter, or a mile long and correspondingly thinner."
>
> "Air is a most powerful electric, and when between the poles of a powerful electro-magnet partakes of the character of a viscous fluid."

Then follows a reference to the usual well-known experiment of moving or spinning copper between the poles, with the following note appended:

> "which shows that air is attracted by powerful magnets and held there with a firm grasp."
>
> "The distinction between electric and magnetic attraction is arbitrary, and no more philosophic than is the distinction between light and heavy bodies, hot and cold bodies, voltaic electricity and galvanism."

The distinction usually drawn between conduction and induction is asserted to be spurious. Heat electricity and magnetism are said to be identical, being all excited by friction. Magnetic attraction may just as well be called gravitation, since the only difference is that it is not necessarily in a vertical direction.

Thus we get led back once more to the subject of gravitation, and Newton's law is replaced by the following foggy statement.

> "Bodies attract each other in proportion to their different states of excitation, in proportion to relative mass, and inversely proportionally to the square of the distance and intervening resistance."
>
> "The point of attraction of the earth for bodies on its surface cannot be in the centre . . . for that would involve the assumption that the opposite half of the globe . . . exerts a force equal to that of the nearer half."
>
> "The theory that attraction is proportional to mass is not borne out by facts: attraction on earth is actually less where the diameter of the earth is greatest. If we turn to the heavens matters are even less satisfactory."

The astronomical determination of the mass of central bodies turns out on their view to be quite illusory.

With regard to "action at a distance," our philosophers find no difficulty whatever; they point out that air and water and other matter only obstruct the fall of bodies, wherefore, of course the less there is between bodies the better they can attract. They quote the usual extract from Newton's letter to Dr. Bentley, and add comments

> "in order to show why such luminaries of the human intellect were unable to see what to us seems as plain as the noon-day sun; and we trust to be able to show the reader that it was again the *ignis fatuus* of suggestive words and false concepts by which these great intellects were decoyed from the path of philosophy into the quagmire of metaphysical word-quibblings."

But now, towards the end of the volume, when the Newtonian theory is once more attacked, the authors fail to keep their mantle so strictly down, and the cloven hoof of the familiar old paradoxer becomes at length conspicuous.

> "This fact, that the planets do not fall into the sun notwithstanding the assertion that they mutually attract

> each other, but, quite contrary to this theory, actually recede from the sun at regular intervals, is as yet an unexplained problem. In works on astronomy, however, the fact is generally glossed over, and it is made to appear as if the recessions were most satisfactorily accounted for. But on closer examination the supposed explanation turns out to be either mere rhetoric or a series of equations of abstract mathematical problems. Why, if sun and planets constantly attract each other proportionally to their quantities of matter, and inversely proportionally to the square of the distance, do they not fall into each other and all into the sun? The question is a perfectly legitimate one, seeing that every writer on astronomy has deemed it necessary to offer explanations. But these explanations are far from satisfactory, as we shall now endeavour to show."

And so they proceed ultimately to arrive at the conclusion that,

> "while there is no doubt that the curvilinear motion of terrestrial bodies is due to the joint action of two forces, viz. the impelling force and the attraction of the earth, there is no evidence either direct or indirect that planetary motions are due to two such independent forces."

The ridiculous ideas capable of being formed by men of so-called education concerning the meaning and function of mathematical reasoning is illustrated on page 353, where it is said first that Newton's achievement was not what is usually supposed:

> "Newton, however, has done nothing of the kind; nor has he even attempted to do so. What Newton did prove was the *proposition* that 'A body [acted on] by two forces conjoined will describe the diagonal of a parallelogram in the same time as it would describe the sides, by those two forces apart.' It is the truth of this mathematical proposition that Newton proved, and not that planetary motions are actually due to two such forces."

And then it is added:

> "Abstract mathematical formulæ can never prove a fact in nature." All that mathematics can do may be illustrated by the case of a man sent to market with eighteenpence, who returns with sixpence, then on the hypothesis that he spent the difference on a dozen apples, mathematics enables us to calculate the price of each apple; or, given the price, it can find the number of apples bought.

Criticism of this sort is gravely printed. It appears to be literally true that many men, including a few schoolmasters, believe mathematics to be represented by schoolboy studies, and especially by "problems leading to simple equations with one unknown quantity."

In Section A, at Toronto, recently, Lord Kelvin implied, *en passant*, that a schoolboy in the very first month of his algebraical studies came across something which when developed led into the heart of the mathematical arcana; viz. when he came across the imaginary roots of a quadratic equation. No wonder that in Lord Kelvin's perspective quadratic equations are relegated in imagination to something like the first month of a boy's mathematical study; but, alas, many of us know boys of sixteen who have been for years at what schoolmasters call mathematics, and who have hardly yet arrived at quadratic equations. In the perspective of many a British school, trigonometry forms a sort of goal, and the elements of the differential calculus loom dim and gigantic in the far future. When feebleness of the kind quoted by the authors is believed to represent anything like mathematics, no wonder that practical men and others regard it with aversion thinly veiled by contempt.

But not only is mathematical reasoning lightly regarded by our authors, they differ from most of their species in treating experiment also in a slight and unsubstantial manner, which though by no means intended for disrespect really amounts to it. A number of experiments are quoted, some of them made apparently by the authors themselves, which if they were true would constitute some of the greatest discoveries of the century; but they are obtained in the most casual manner, as if, like the scientific hero of many novels, the authors had only to retire into a back room for about twenty minutes, in order to make the most momentous and fame-bringing discovery. We quote two instances.

> "By heating a body it is made to weigh less: that is, not merely is its specific gravity lowered, but its absolute weight is less, and it regains its former weight on cooling" (p. 380).
>
> "Our explanation of this is that, though cool to touch, the molecules are still in a state of excitation, and hence their lesser weight" (p. 383).
>
> "Another experiment was made as follows. A glass tube sealed at one end was contracted in the middle. In the lower portion were placed about 10 c.c. of water, and in the upper portion a stick of dry potassium hydrate of about an equal weight, and the tube was sealed. After cooling, the tube was weighed and then turned upside down, so that the water could flow on to the potassium hydrate. The stick of potassium hydrate partially dissolved, and the solution crystallised. On weighing it was found to be lighter by about 20 mgr. On shaking the glass the crystals partially redissolved, and the tube became heavier; but after some time the crystals re-formed, and the tube weighed again less" (p. 384).

On pp. 397 and 401 we are told that inside the crust of the earth there is a neutral sphere or zone outside which bodies press inwards, but within which they press outwards, so that gravity at a certain depth in the earth is inverted and bodies press upward. So this naturally explains volcanoes,—in fact the earth, if cooled, might explode like a Rupert's drop (p. 402).

Matter from the interior, if it ever got above the neutral zone, would be found to be light instead of heavy, and hence it is that the dust of Krakatoa took so long to settle (pp. 400 and 407)!

After this we can be surprised at nothing, and so we go on to learn:—

That the axial revolution of the earth is due to the radiation of the sun, like the motion of a radiometer or of a sunflower.

That the real diameter of the earth's orb is 506,734, and not 8000, miles.

That by reason of its axial rotation the earth rolls on its own orb, pressing against the zone of neutral attraction, and thus effecting its annual journey round the sun like a coach-wheel.

That the sunspots are circumsolar planets with satellites, their apparently irregular shape being an easily explained optical illusion.

That the terrestrial seasons are caused by unequal solar attractions; and other extraordinary nonsense.

We will on these topics make only two quotations, though the exposition of the doctrines occupies several chapters.

> "Nobody will question that at a certain distance between sun and earth a body will be more strongly attracted by the latter than by the former, or by the former than by the latter, as the body may happen to be nearer the one or the other. In other words, nobody will question that the earth has its own field of attraction as against the sun; hence all that is within that field of attraction would form part and parcel of the mass. Now, the distance between earth and sun is at all times determined by their relative states of excitation, as before explained. Hence the earth may be regarded at any one moment as being kept at a certain definite distance from the sun, as if held there by ropes or bars. But the earth revolves, and in revolving meets with a greater resistance on the side nearest the sun than on the opposite side; hence there is greater retardation on the one side than on the other: from which follows the translation in orbit. The laws of rigid mechanics find, therefore, here application. The earth is drawn towards the sun, but cannot pass a certain line; *and this line is the rigid surface against which it presses.* To the eye nothing may be there impenetrable; but to the earth this invisible circle is like a hoop of adamant, against which it presses and along which it is rolling in space."

And this brief note:—

> "Prof. Helmholtz estimated the depth of the luminous envelope at 500 (!) miles. This estimate . . . is ludicrous."

Thus at length the lofty position appropriate to superior knowledge is fully assumed; and from pitying and apologising for Newton, rebuking and correcting Thomson and Tait for their fundamental errors, the authors proceed to laugh at an estimate by von Helmholtz.

Behold, a greater than these men has arisen; another still larger volume already in manuscript is threatened; and the natural philosopher of the twentieth century is to be Ignatius Singer! O. J. L.

PHILOSOPHY OF KNOWLEDGE.

Philosophy of Knowledge. By Prof. G. T. Ladd. Pp. xv + 614. (London: Longmans and Co., 1897.)

PROF. LADD, of Yale, is well known as an industrious writer on psychology, and upon philosophy regarded mainly from a psychologist's point of view. The present volume gives his Theory of Knowledge, and with some *naïveté* he claims for it "the treatment due to a pioneer work," and avows a "quite unusual interest" in its success. His method he characterises as a constant striving "to make epistemology vital—a thing of moment, because indissolubly and most intimately connected with the ethical and religious life of the age."

Most modern psychologists of note have definitely broken with that abstract and detached view of their science which would argue the question of the "origin" of knowledge within the limits of a narrow subjectivism, and which would assume that thereby the questions of nature growth and validity had received adequate solution. The present tendency of psychology is rather to offer itself as that specialist training which makes general metaphysic profitable, or, at any rate, to endeavour to set itself right with metaphysic, by an alliance based, perhaps, upon a compromise.

Of such a tendency Prof. Ladd is a representative. He puts forward a strenuous plea for an ultimate view of knowledge and reality which shall neither be subjective idealism nor crude realism, neither wholly dualism in one sense of that word, nor wholly monism in one sense of that word. He insists on the implication in the act of knowledge of what he not very happily calls "extra-mental" or "trans-subjective" reality, meaning that we apprehend that which is manifestly independent on our individual consciousness, and does not emerge in it as a a matter of course as the result of its own laws. He urges the claims of our feelings and our will to be satisfied equally as well as our thinking taken abstractly. He demands as a sort of postulate of faith that we take it that things are known in some sense as they are. The self, he holds, is known as it really is, thinking, feeling, and willing. And things are known as what will not always as we will, and by an analogical *saltus* as other will.

This is interesting speculation, but it is rather eclectic than original. Prof. Ladd would seem to be a pioneer only in the sense that we are at present fortunately free from *Erkenntnisstheorie* as it is too often understood in Germany. For he is something too well read in theories of knowledge which pass with undue freedom from psychology to metaphysic, and often approach perilously near to *ignoratio elenchi.*

It is in his criticisms that this influence, which constitutes, as we venture to think, the weakness of his work, makes itself apparent. Interlined with positive views as to the relation of faith to knowledge, of æsthetic and ethical value to reality, where Prof. Ladd obviously owes much to Lotze, or as to the relation of thought and reality to will, where Wundt's influence is manifest, there are offered an erudite though inconclusive history of opinion and a running fire of uncomplimentary comment upon Kant, Mr. Spencer, Mr. F. H. Bradley, and others. And the sympathetic insight of the successful critic is wanting to the Yale professor. The results, as well as the methods, of certain thinkers are agnostic or sceptical, and that must not be. And so a discursive appreciation of various points of their doctrine is put forth, which, *e.g.* in the case of Kant, ranges, with the exception of a suggestion as to the implication of will in the treatment of the second analogy of experience, entirely within the circle of commonplaces on the subject, and is altogether unconvincing to believers in the results of sympathetic interpretation. Again, in dealing with post-Kantian idealism, it is not its alleged panlogism which he criticises, as he might well have done from his Wundtian standpoint, but rather its general tone or points so truistic that any theory of knowledge must, and if not explicitly yet tacitly does, meet them.

The discursive and rhetorical style of the book would of itself tend to ineffectiveness in matters of criticism. It is often picturesque, and has many happy phrases; it sometimes rises to eloquence, and is always eminently readable. But it is singularly vague and elusive. And the volume needs condensation. H. W. B.